Landscape Ecol (2008) 23:7–10
DOI 10.1007/s10980-007-9180-9

PERSPECTIVES

Modeling tsunami damage in Aceh: a reply

Louis R. Iverson · Anantha M. Prasad

Received: 19 October 2007 / Accepted: 29 October 2007 / Published online: 24 November 2007

Abstract In reply to the critique of Baird and Kerr, we emphasize that our model is a generalized vulnerability model, built from easily acquired data from anywhere in the world, to identify areas with probable susceptibility to large tsunamis—and discuss their other criticisms in detail. We also show that a rejection of the role of trees in helping protect vulnerable areas is not justified in light of existing evidence.

Keywords Coastal forests · Tsunami vulnerability · Random Forest · Aceh · Mangrove

We thank Baird and Kerr for their critique of our paper "Using landscape analysis to assess and model tsunami damage in Aceh province, Sumatra": (Iverson and Prasad 2007). We believe it is indeed healthy to continue scientific dialog on such an important issue so that the science may build with greater consensus. We wish to continue that dialog with this reply to their concerns.

L. R. Iverson (✉) · A. M. Prasad
Northern Research Station, United States Department of Agriculture, Forest Service, 359 Main Road, Delaware, OH 43015, USA
e-mail: liverson@fs.fed.us

A. M. Prasad
e-mail: aprasad@fs.fed.us

Baird and Kerr's primary criticism was that "they do not justify their conclusion that tree belts provide an effective defense against the Indian Ocean Tsunami in Aceh." Their second point was that our model "ignores many variables known to be important in the area studied." We will address these in reverse order.

It is unfortunate that the main purpose of the paper was mostly missed by the Baird and Kerr. Our paper emphasized that by using free modeling software and free or nearly free data, it is possible to prepare a simple but reliable map of vulnerability to tsunami for anywhere on the planet. We have used the detailed information available from the aftermath of the 2004 tsunami to build a general model for mapping *vulnerability* to an extreme tsunami, not actual damage where it occurred in Aceh. We purposely wanted a simple model, readily transportable and not relying on specialized data or expertise to mechanistically model localized variables involved in tsunami damage. Though we were heavily criticized for not including such an important variable as distance from the epicenter, we naturally could not include this variable because in a general model for assessing tsunami vulnerability, no one can know where the next epicenters will be. Despite the fact that our model is simple, using only four (not three as the reviewers claim) variables, and deliberately excluding coral reefs, bathymetry, or distance from epicenters, the model works (94% of the damaged area correctly classified). The Kappa statistics classified the match between model and actual as 'excellent', and our modeled estimate of damage for the 2,000 km^2 study area was 435 km^2 vs. 450 km^2 actually damaged. And this model runs at a spatial resolution of 30 m. We eagerly await the day when more sophisticated and successful models can be built providing detailed mapping of vulnerable areas for all coastal zones in the world. In the meantime, we offer a simple approach that can be applied anywhere to give some baseline to vulnerability to killing tsunamis.

The reviewers legitimately criticize us for the lack of detail on the 'damage' variable that was used as our response variable in the model building. Unfortunately, we cannot know all the details, nor could we disclose them if we did. These data were acquired by the senior author while working for the U.S. Agency of International Development. These data were readily available to anyone working on the tsunami, but the details on the acquisition were withheld by the U.S. Government, National Geospatial-Intelligence Agency. They are now available through the Pacific Disaster Center (http://www.pca.org). The data were distributed as 69 polygons ranging in size from 1.2 to 45,187 ha. We have confidence that the analysis of damage versus nondamage was well done for all vegetation types, and that the forest need not be wiped out to be called 'damaged.' This level of detail was useful to build a reliable model, but is not needed to extend the model elsewhere to prepare general vulnerability maps. Please contact the authors if you wish to apply our Aceh-derived Random Forest model to your region of interest.

As for importance of variables, we were criticized for putting "an emphasis on vegetation." As landscape ecologists studying forested ecosystems we admit to being a little guilty on overemphasizing forests in our abstract. However, we clearly state that the Random Forest importance scores were 0.1647 for elevation, 0.1643 for distance from shore, 0.1589 for vegetation type, and 0.1354 for exposure (Table 2, Iverson and Prasad 2007). We recognized in the paper that elevation and distance from shore were the primary variables, just as others do, including the reviewers (Kerr et al. 2006; UNEP-WCMC 2006). But our evidence also supports that vegetation also is an influencing factor. Just because it doesn't surface as the first variable doesn't mean it isn't important or that it has "no mitigating effect" as the reviewers claim. We emphasized vegetation because this is one tractable variable humans can work with.

The reviewers are correct that in our paper, "there is no direct test of the effect of tree belts in mitigating destruction of developed areas." We concede that our evidence is not a direct test. Meanwhile, we have some indirect evidence. First, forest/nonforest vegetation was the third most important variable in the RF model, fairly close behind distance from shore and elevation. Second, we found a 2.5 times greater likelihood of damage in developed areas versus forested areas within 2 km of the coastline. Third, there were substantially higher ratios of damaged to undamaged land for developed areas over forested land within 2 km of coastline and below 20 m in elevation. We were working with 187 km of coastline and found that, within that low elevation zone, the ratio of damaged to undamaged land was 40% higher for the 124 km^2 of developed versus the 122 km^2 of forested land within 2 km of coast. An analysis of the spatial configuration of these areas may provide more direct evidence of the protective value of forests within this zone.

We cannot understand how the reviewers can say that the Danielsen et al. (2005) has been "widely discredited' or that the current literature found "no mitigating effect by vegetation." The Danielsen et al. (2006) response to the critique by Dahdouh-Guebas and Koedam (2006) included convincing quantitative data supported the beneficial effect of vegetation (see Danielsen et al. 2006, Supplementary Table S1). In the Andamans, less erosion was observed behind mangroves as compared to no mangroves (Department of Ocean Development 2005). Dahdouh-Geubas et al., (2005) found that for 24 lagoons and estuaries along the coast of Sri Lanka, there was little destruction to the coast if the mangroves were of high quality. Several other reports show the benefits of mangroves in mitigating effects from the tsunami (e.g., Bambaredeniya 2005; Harakunarak and Aksornkoae 2005; Kathiresan and Rajendran 2005, 2006; Sudmeier-Rieux et al. 2006). There are many other observational reports of mangroves reducing damage and saving lives by preventing people from washing back to sea (UNEP-WCMC 2006).

We were criticized for not including the most recent literature, such as Chatenoux and Peduzzi (2007), that was purported to provide "much to refute" the mitigating role of forests. Upon reviewing this paper (please note that it came out many months after ours was in press), we find no refutation of the findings of any of the papers quoted above. The abstract states "The mangroves forests identified in the study were all located in sheltered areas, thus preventing to address the potential protecting role of mangroves forests"—meaning they could not evaluate the role of mangrove forests and hardly a strong indictment of all the evidence supporting their protective role.

Even if the reviewers are correct that protected forests would be insignificant for dampening the full force of a tsunami of the size of the December 2004 tsunami, it is not contested that such forests can mitigate the effects of storm surges and wave damage (Mazda et al. 1997; Massel et al. 1999; Danielson et al. 2006); thus planting and protecting such forests are extremely important regardless of their role in tsunamis.

The reviewers assume that we are making a "pitch for tree belts without any thoughts of consequences." Let it be clear that we do not and are offended by this assertion—we unequivocally agree that people should not be dislocated from low-lying coastal areas without the appropriate attention to social justice issues, by pressures due to commercial interests, or in preference to putting an excellent tsunami early-warning system in place. We regret if any interpretation contrary to this could have been inferred from our study. While we concede more research is needed, we believe that there is currently enough evidence that judicious, case-level placement and integration of forests in the most vulnerable locations is justified provided there is democratic participation of the people affected (Barbier 2006). This would be beneficial not just as future insurance against tsunamis but also against hurricanes, sea-level rise, storm surges due to climate change, etc. (Adger et al. 2005). Such forests also provide a whole host of ecosystem goods and services, including food, fuel, recreation, and shelter material for humans, as well as habitat for a diverse group of organisms.

To repeat, the main point of the paper is that vulnerability maps to large tsunamis can be prepared for any coastal region in the world. This approach using four terrestrial variables and the Random Forest modeling technique proved effective in Aceh and should work well elsewhere.

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